

The bulbiferous form is the most frequent in this vicinity, but we do not believe that the environment plays any rôle of importance, since we have found this form in sandy soil at the border of pine woods as well as in deciduous woods with rich humus.

None of the species of the sections *Pterodes* and *Anthelaea* exhibit this structure; it is confined to *Gymnodes*. In *Luzula nodulosa* (Bory) E. Mey. of the last section the rhizome is moniliform, but tuberous, not bulbiferous; similar tuberous rhizomes are known from several species of *Juncus*, for instance *J. nodosus*, *J. marginatus*, etc., while bulbs or bulb-like formations, known in the inflorescence of various species of *Junci septati* and *graminifolii* are caused by insects, *Livia* for instance.

CLINTON, MARYLAND.

ON SOLIDAGO RIGIDA L., AND THE APPLICATION OF OLD BOTANICAL NAMES.

C. A. WEATHERBY.

Mr. Mackenzie's recent article on *Solidago rigida*,¹ in which he concludes that that name belongs to the species generally known as *S. patula* Muhl., furnishes a striking illustration of the uncertainties into which we are likely to be led by a strictly historical—one is tempted to say archaeological—method of determining the application of Linnaean and other old botanical names. For the case is by no means so clear to others as it appears to him.

It may be that the description of *Solidago rigida* in the *Species Plantarum* is not to be regarded as original there, though Linnaeus, in making it up, did revise the quoted diagnosis from the *Hortus Cliffortianus* to the extent of adding the word "scabris"; nor as based on the specimen in the Linnaean herbarium. But that specimen may have been part of the basis of the undoubtedly original description in the *Hortus Cliffortianus*; Linnaeus received "all the duplicates of the Clifford herbarium"² and for aught Mr. Mackenzie tells us, this may have been one of them. It might have been well to determine this point before altogether rejecting the specimen as representative of the species.

¹ RHODORA xxviii. 29-31 (1926).

² See Jackson, Proc. Linn. Soc. cxxiv, suppl. 11 (1912).

In any case, Linnaeus had certainly seen a specimen when he wrote the description in the *Hortus Cliffortianus*—a specimen “ramis . . . fastigiatis, corymbis terminatricibus.” A blind man feeling with his fingers could hardly apply these terms to the strongly racemose inflorescence of *S. patula*, in which the lower branches tend to be remote and spreading and all are almost always floriferous at least to the middle and often nearly to the base. On the other hand, the phrases of Linnaeus apply well to *S. rigida* as currently interpreted, in which the branches tend to be ascending and crowded toward the summit of the stem and are only exceptionally floriferous for as much as half their length, usually bearing corymbiform inflorescences at or near their apices. And a manuscript note of Dr. Gray’s states that the specimen of “*Solidago foliis caulinis ovatis*” etc., in the Clifford herbarium is actually *S. rigida* of current manuals.

There can, then, be little or no doubt that *S. rigida*, in its traditional sense, is an element in the *S. rigida* of Linnaeus. Are we, by the historical method, to regard the Clifford specimen as the type, to be associated with the original description of the *Hort. Cliff.*, with which Linnaeus placed as a synonym Herman’s “*Virga aurea novae angliae, lato rigidoque folio*” (erroneously, if Herman’s plate really represents *S. patula*); or are we, following the principle of the name-bringing synonym, to take Herman’s plate (always supposing that it represents *S. patula*) as representative of the species, and the Clifford specimen and description as erroneously associated with it by Linnaeus? In the similar case of *Limodorum tuberosum*,¹ Mr. Mackenzie chooses what corresponds to the former alternative; in this instance, he adopts the latter—without, as it appears to me, arriving at any conclusive results.

Herman’s plate² is “excellent” in the technique of engraving; but it is not an accurate representation of either of the two species concerned. The lower leaves will do very well for *S. patula*; but the strongly ascending branches, beset with broad-based leaves and bearing terminal corymbs of flowers are quite wrong for that species. It is as if the artist had combined the two. Herman’s description is similarly unsatisfactory; much of it seems to apply to *S. patula*, but the statement that the leaves are “as if embracing the stem at base” is at least greatly exaggerated for that species, but accurate enough

¹ RHODORA xxvii. 193–196 (1925).

² *Parad. Bot.* t. 243 (1705).

for many specimens of *S. rigida*, in which the broad, subcordate bases of the leaves do appear amplexicaul. And finally the phrase "tactu aspera" on which Mr. Mackenzie so much relies, applies to either species; the leaves of *S. rigida* are also strongly scabrous, most so beneath.

More might be said, but the above should be enough to show in what an unsatisfied state we are left by the method of determining the application of the Linnaean name followed by Mr. Mackenzie. Yet all the while we have before us two perfectly definite facts—the specimen in the Linnaean herbarium, concerning the identity of which there is no doubt, and the unanimous interpretation of the species by (to go no further) Michaux, Pursh, Eaton, Torrey, Gray, Wood, and Britton.

The verticillate *Eupatoria* present a similar picture. They have been studied by Wiegand¹ and Mackenzie.² The two agree perfectly as to the taxonomy of the group, but disagree just as completely as to the application of the three Linnaean names concerned. Repeated rereading of their papers in an effort to arrive at some conclusion satisfactory to me, has convinced me that one argument is, as an argument, precisely as good as the other. Each has its strong and weak points; each is based partly on assumption. We are left with the necessity, if we wish to be certain of being understood, of labelling every specimen in the group with two names, one according to Wiegand and one according to Mackenzie. Yet, here again, there are in the Linnaean herbarium specimens of all three species, which were in his possession when the original portions of his descriptions were written. They could probably be identified by a botanist familiar with the rather technical species involved; so identified and accepted as types, they would at once end the present annoying and hampering ambiguity. Surely, in cases like these, the concrete fact of the Linnaean specimen should be allowed to decide what is otherwise incapable of decision.

It might, indeed, be plausibly argued that, if we were really willing to accept the rule that botanical nomenclature begins in 1753 as meaning what it says, specimens in Linnaeus' possession in that year would a priori become types of his species, regardless of the nomenclatorially invalid pre-Linnaean citations with which he may have

¹ RHODORA xxii. 57-70 (1920).

² Op. cit. xxii. 157-165 (1920).

associated them.¹ I am not, however, advocating anything so extreme and so liable to produce its own crop of confusion; as has repeatedly been pointed out,² Linnaean specimens are often not types in the modern sense; and, what is more important, it is in the highest degree doubtful if any rule can be devised which will work satisfactorily in all cases or even the majority of them. The circumstances are so various that each is best judged on its own merits. But there are certain considerations which can always and profitably be borne in mind.

In the first place (and this is quite generally forgotten) botanical nomenclature is not in itself primarily a subject for scientific investigation, or a branch of historical research, but a practical device, closely analogous to language. Like language, it has (aside from the work of a few nomenclatorial theorists) assumed its present form through the gradual and more or less spontaneous growth of a body of usage; like language, it is useful just in proportion to the degree in which its component units are employed by everyone in the same sense. The amateur speaks from a sound instinct when he protests against change, even though he has commonly little understanding of the technical difficulties which confront the professional. It is a genuine misfortune when names like *Quercus rubra* and *Solidago rigida*, which for a century and three-quarters have been so consistently and accurately used in a given sense that they have not even accumulated an appreciable synonymy, are shifted from their well-nigh immemorial applications. One could wish that in such cases the principle of the nomen conservandum could be extended to specific names and made to include their application as well as the names themselves.

But this principle would not often have to be invoked if we would bear in mind a second simple consideration—that, if we are ever to achieve stability in the application of old names, we must cling wherever possible, to what is definite and indisputable, and flee from what is merely interpretative, conjectural, and open to argument. The concrete fact may be, as in the above cases, a specimen; it may be a good plate; it may be simply established usage. Very often the best way to arrive at stability is merely to let well enough alone; that

¹ This, of course, would reverse the method—of the name-bringing synonym—generally employed for post-Linnaean literature; but it would make 1753 a real starting-point, not, as is the tendency at present, a more and more insignificant milestone on the road of nomenclatorial history.

² See, for instance, Blake, RHODORA XX. 21 (1918).

is, to follow usage unless we can certainly prove it wrong. And by wrong, I do not mean inconsistent with our own individual conceptions of an ideal nomenclatorial system, or with our opinions as to the technical application of existing rules, but demonstrably mistaken.

I am not, of course, arguing against all change; unless we are to become fossilized, change there must be, until our knowledge is perfect. But I am protesting against unnecessary change; and in particular against shifts in typification made on the basis of inconclusive bibliographical investigation, or on any other basis except irrefragable evidence of error.

The above considerations apply with double force to generic names, where a shift may affect, not one only, but many combinations. Here the makers of the International Rules did wisely in leaving the matter of the typification of genera rather vague. They doubtless realized that in many instances it had, for all practical purposes, been already accomplished; in others, it could be hoped that the conclusions of competent monographers would in time win universal acceptance. Is it practicable to go much further?

I look forward with misgiving to the ultimate consequences of the rules for the typification of genera contained in the American Code and in the Type-Basis Code, which it is now proposed to incorporate in the International Rules. Both, at least in their published applications, show the same readiness as does Mr. Mackenzie to pursue the chronologically primary element into the hazy and amorphous distances of pre-Linnaean literature, with the same probability of inconclusive results. Both present so complicated a series of alternatives that there is almost certain to be difference of opinion as to their application in any doubtful case. Both have to deal with the inherent difficulty of imposing the modern notion of types on the work of old authors whose heads it never entered. Both, therefore, may be expected to produce a high percentage of uncertainty and argument—and of these commodities we have enough already.

Probably, stability in generic nomenclature would most surely be approximated by the use of a method like the following.

1. The *Species Plantarum* should be made a real starting-point. All the genera of Linnaeus should be accepted as there constituted, regardless of the source from which he took their names, or of the sense in which these names were used by the authors from whom he took them. All species in a given genus should be regarded as equally

eligible to serve as the generic type, unless very definitely excluded by collateral circumstances.¹

2. Where a genus has been later divided, the application of the name should be determined simply by current majority usage, as expressed in standard works. Should these prove hopelessly at variance, some of the provisions of the American Code might be employed as a secondary means of arriving at a conclusion.²

In other words, the typification of Linnaean and other old genera should be done, not at the beginning of their history, as is now being attempted, but at the end, after the accumulated results of taxonomic work have been taken into consideration.

Such a method would not be arbitrary, in any proper sense;³ on the contrary, since it would endeavor to follow the lines of the actual development of nomenclatorial practice, it would be much less arbitrary than the setting up of *ex post facto* rules. It would be simple and at least apparently practical. It would enable us at once and without further argument, to retain such names as *Pteris* and *Pteridium*, *Holcus* and *Sorghum*, *Aira* and *Deschampsia*, *Sisymbrium* and *Erysimum*, *Leontodon* and *Taraxacum*, and even *Gerardia*, in their traditional senses.⁴ And it would support the sound principle

¹ As, for example, certain species of *Acrostichum*, like *A. platyneuron* and *A. polypodioides*, which belong to other Linnaean genera; or, perhaps, species directly designated by Linnaeus as exceptional in their genera.

² When once the group to which the generic name is to belong has been thus determined, the choosing of an individual type species, if thought necessary, becomes of little importance and might be accomplished by any practicable means. In many cases, the type would automatically choose itself, through there being but one Linnaean species in the segregate genus.

³ There is a certain tendency to use this term as a generally derogatory adjective, applicable to any system but one's own.

⁴ For shifts in the application of these names see Britton, *Flora of Bermuda* 419 (1918): *Illustrated Flora*, ed. 2, ii. 162, 173, iii. 315 (1913); Mackenzie, *RHODORA* xxvii. 28, 47, 65 (1925); Hitchcock, *Am. Journ. Bot.* viii. 253, 255 (1921). x. 512 (1923). It may here be remarked that the interpretations of the International Rules given by Hitchcock and Mackenzie appear to me, in the light of the examples cited in these rules, wholly without authority. These authors simply read into the rules their own ideas as to typification. For instance, Art. 45 of the International Rules provides that when a genus is divided, if it contains a section or some other division which, judging by its name or its species, is the type or origin of the group, the name is reserved for that part of it. The single example given is as follows: "the genus *Helianthemum* contained, according to Dunal (in *DC. Prodr.* I. 266–284 [1824]), 112 well-known species distributed in nine sections; several of these sections have since been raised to generic rank (*Fumana* Spach, *Tuberaria* Spach) but the name *Helianthemum* has been kept for the divisions grouped round the section *Euheliantthemum*." It is indeed hard to find in this example, expressly chosen to illustrate the meaning of Art. 45, any justification for Hitchcock's argument that the "historic type" of *Panicum* is *P. italicum* L., and that the name *Panicum* should therefore be applied to *Setaria* under the International Rules (*Am. Journ. Bot.* viii. 252–253).

that the judgment of our predecessors, on whose work our own is necessarily based, expressed in usage, is not to be summarily thrust out of court, as by the American Code, or given incidental and subordinate consideration, as by the Type-Basis Code, but to carry its due influence.

It has been objected that usage is variable and hard to determine; but this difficulty is, I believe, more apparent than real. The familiar names are not always those that *we* learned;¹ they may be, as in the case of *Solidago rigida*, also those which our botanical fathers, grandfathers, and great-grandfathers learned before us. As to current usage, I have before me five representative European manuals—Bentham & Hooker's *British Flora*, ed. 7; Bonnier & de Layens's *Flore de France*; Archangeli's *Flora Italiana*, 2nd ed.; Schinz & Thellung's *Flora der Schweiz*, ed. 4; and Garcke's *Flora von Deutschland* ed. 22. Representatives of eight of the ten genera listed above as the subject of recent shifts under the American and Type-Basis codes appear in all of these floras. There are differences among them as to generic limits; two do not recognize *Deschampsia* and only two take up *Pteridium*. But whenever a genus is recognized at all, it appears under the same name and contains the same nucleus of Linnaean species—is used, that is, in precisely the same sense. The same is true of the American manuals at hand up to the publication of the second edition of the *Illustrated Flora* in 1913; I have no doubt it would prove true if the genera concerned were followed back at least to the *Prodromus* and Kunth's *Enumeratio*. In these cases, and in hundreds of others, usage has become crystallized and definite; what advantage can there be in disturbing achieved definiteness, or in opening the way to disturbing it, merely to satisfy a theoretical and largely untried system?

In 1896, Ascherson & Graebner wrote, in the preface to their *Synopsis der mitteleuropäischen Flora*, "We hold that nomenclature should be considered, not as an end in itself, but only as a means to the end of the widest possible intelligibility; and that therefore there

Similarly, his citation of Art. 19 as supporting his typification of *Holcus* overlooks the fact that this article has nothing whatever to do with typification, but was intended simply to legalize the adoption of the genera of the *Species Plantarum*, which were there published without the description required by the rules. (See Art. 38).

¹ Cf. Hitchcock, *Am. Journ. Bot.* xiii. 291 (1926).

is no question of principle in regard to it, but only of expediency."¹ These are wise words; they might well be inscribed above the chairman's seat in any assembly where botanical nomenclature is discussed.

EAST HARTFORD, CONN.

TWO SUMMERS OF BOTANIZING IN NEWFOUNDLAND.

M. L. FERNALD.

(Continued from p. 129.)

PART III. NOTEWORTHY VASCULAR PLANTS COLLECTED IN NEWFOUNDLAND, 1924 AND 1925.

THERE is no satisfactory list of Newfoundland plants, and the detailed *Flora* which Professor Wiegand and I began in 1910 still needs so many finishing touches that it cannot now be presented. During the past two seasons, however, so many plants have been found which are new to the flora either of Newfoundland or of eastern America that their orderly enumeration at this time is appropriate; and, in order to determine the exact identities of some species, detailed revisions of certain groups have been necessary. In so far as these revisions are completed they are here presented; but certain groups, highly developed in the flora of Newfoundland, still await more critical study and reports upon them must be deferred. These include, among others, the genera *Poa*, *Polygonum* § *Avicularia*, *Cochlearia*, *Euphrasia*, *Campanula* and *Taraxacum*. In some cases, where new northern or southern limits in Newfoundland have been established, it has seemed appropriate to note species already known from remote sections of the island; and in a few cases new species are described or combinations made for extra-limital plants which have come to my attention in studying those of Newfoundland.

WOODSIA ALPINA (Bolton) S. F. Gray. Dry limestone cliffs, western face of Doctor Hill, and calcareous escarpments, western face of Bard Harbor Hill, Highlands of St. John, *Fernald & Long*, nos. 27,203, 27,204. See pp. 124, 125. Earlier collections only from Notre Dame Bay.

¹"Sind wir . . . der Meinung, dass die Nomenclatur stets nur als Mittel zum Zweck der Verständigung in möglichst weiten Kreise, nicht aber als Selbstzweck betrachtet darf, and dass es dabei nur Zweckmassigkeits-, nirgend aber Rechtsfragen gibt."